Letters to the Editor.

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The Apparatus of Dr. Russ.

I HOPE Dr. Hartridge will pardon my suggestion that he is dismissing the possible effects of temperature too lightly.

In the Phil. Trans. Roy. Soc. of 1792, p. 86, Mr. Bennett described apparatus of great sensibility, in which a piece of dragon-fly wing or thistledown carried on a light arm suspended by a spider line in a closed case responded with amazing sensibility to the heat from a person at a distance in virtue of convection currents set up by the warmer side of the case. It is not surprising that suggestions of animal magnetism should have been made, *e.g.* that the right hand should act oppositely to the left, but the author of the paper ignored these, and was content with explanations based upon known laws of physics.

A little later (1798) Cavendish in his famous paper on the mean density of the earth showed how potent minute differences of temperature were to disturb even the 2-in. balls of lead that he suspended from the ends of his lever.

In 1862 Joule described in the Proc. Lit. and Phil. Soc. Manchester, p. 73, a convection thermometer in which a glass tube 2 ft. long and 4 in. in diameter was divided longitudinally into two portions by a diametrical partition extending to within about 1 in. of the top and bottom. "In the top space a bit of magnetised sewing-needle, furnished with a glass index, is suspended by a single filament of silk." The draught up on the warmer side and down on the cooler side caused the needle to be deflected, acting on the glass index as a wind-vane. This was found to be a superlatively delicate radiation thermometer.

In 1890, in conjunction with the late Dr. Watson and Mr. Briscoe, I showed to the Physical Society (*Phil. Mag.*, 1891, p. 59) an experiment which increased the delicacy of the Joule thermometer very greatly by replacing the straw and silk by a mirror and counterweight hung by a quartz fibre, but we found that by no system of screening, even in a cellar, could we maintain such quiet in the air as to allow the mirror to remain anywhere near the neutral position or to remain at rest. One side or the other was the hotter, and this was always changing. If we had never succeeded in obtaining a real state of rest the delicacy would have been useless. We, however, hit upon a plan which did keep the two sides strictly alike in temperature. We surrounded the tube by an exterior glass tube kept turning on its axis rapidly by clockwork all day. As the exterior glass was opaque to "dark heat" and no light was allowed to fall on the tube, the inner tube could not have one side hotter than the other, and then the mirror came to its neutral position and was very fairly steady there, so that heat developed electrically on one side of the partition in warming the air gave rise to deflections which could be measured with some certainty.

In all the delicate work that I have done with quartz-fibre suspensions the strictest attention to freedom from disturbance by air movement was essential to success. Only by such special care can air movements of so small an amount as 1 in. in a fortnight or so be avoided, and if not avoided a stable zero on which everything must depend is impossible.

In no ordinary large apparatus of the physical laboratory is the air ever quiet, and in the closed box of Dr. Russ is it safe to suppose that there are not convection currents abundantly able to cause deflection of anything suspended by a single silk fibre?

C. V. BOYS.

The Designation of the Radium Equivalent.

IN all problems that are primarily concerned with strictly radio-active phenomena the quantity λN, denoting the number of atoms transformed in a unit of time, plays a very important part. In such problems comparable amounts of different radio-elements are such as correspond to the same value of λN. There is need for a name to denote the amount of any radio-element, irrespective of family, that is thus comparable to one gram of radium. If, tentatively, we use the letter r to denote this desired name, then an r of any material may be defined as that amount of the material that will produce transformed atoms at the same rate as transformed atoms are produced by one gram of radium. The quantity r plays in radio-activity a part that is analogous to that played by the gram-molecule in physical chemistry, and the advantages to be secured by naming it are quite similar to those that were secured by the introduction of the term "gram-molecule."

As the curie is an r of radium emanation, the adoption of a new name to denote the quantity r will give two names for the same quantity of radium emanation. The majority of those with whom the subject has been discussed regarded this as undesirable. They consider it better to redefine the curie so as to cover the entire field.

I shall be glad if you will publish this letter so that a further expression of opinion may be obtained. A more detailed presentation of the subject will shortly appear in the Journal of the Washington Academy of Sciences.

N. ERNEST DORSEY.

Bureau of Standards, July 30.

Pisidium clessini in British Lochs.

DR. ANNANDALE (NATURE, August 18, p. 778) assumes from Mr. B. B. Woodward's letter that this species is a deep-water form, but this is not so. *P. clessini* is abundant in some of the Welsh and Kerry tarns, where Mr. Charles Oldham and I have collected it in from 1 to 4 ft. of water. It is a form which I have had under observation for some years past—indeed, since I first collected it on Brandon Mountain in Co. Kerry in 1910.

Not being able to identify it with any described species of Pisidium, I have several times been on the point of figuring it as new. At the last moment, however, I have always been checked by the fact that I was not satisfied that it was a good species. This view, I may say, is shared by my colleagues, Mr. Charles Oldham and Mr. R. A. Phillips. We are not satisfied that *P. clessini* is anything but a cold-water (depauperate) form of the widespread *P. casertanum*. This latter occurs abundantly also in many mountain tarns, but is always—in our experience—conspicuous by its absence in those in which *P. clessini* occurs.

Superficially, *P. clessini* is very distinct, and Dr. Odhner is satisfied that its anatomical characters render it necessary that we should regard it as a species; yet he has not been able to assure me that these characters are not the result of starvation acting over a prolonged period on a number of generations. For my own part I shall not be satisfied in its standing as a species until I can find it living in association

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with *P. casertanum*. In Cwm Clyd on Y Garn, in North Wales, I have taken *P. casertanum* within a few yards of *P. clessini*, but the former was in a shallow swamp which warmed up in the summer, while the latter was living in the very cold water of Llyn Clyd. A. W. STELFOX.

National Museum, Dublin.

Scientific Publication.

THERE is at present much discussion of the difficulties of scientific publishing, and such discussion with resultant action is necessary, for there are few, if any, signs of reduction in printing costs, and the output of manuscript is steadily increasing. Societies issuing journals cannot meet their expenses unless the present high subscriptions are maintained, or in many cases increased, and most of us can with difficulty withstand the present drain upon our resources, and certainly cannot afford another penny in this direction. There would seem to be three alternatives before us. One is to ask each author to pay for or subsidise the publication of his own paper. This in many cases is financially impossible, and in any event introduces a whole series of new and very difficult problems, which renders it an extremely undesirable solution. The second is to reorganise our scientific societies and publishing boards with a lumping together of transactions, journals, annals, etc., and a consequent cheapening of direction and production. This is, to my way of thinking, the obvious solution, but few seem to agree thereon. The third method is to make a radical change in the format of our scientific journals, with or without an alteration in the existing structural relations of the learned societies. This is the substance of the present letter.

Perhaps there are two main reasons why people write scientific papers: first, that the authors really believe that their work will help forward the progress of science; and, secondly, that, having spent one or more years investigating a problem, they naturally wish to justify their time to themselves and their colleagues, to keep their names before the scientific public, and to give a basis for promotion in the scientific hierarchy. The memoirs are very stereotyped, being written in moderate detail with selected, and occasionally digested, original matter, and rounded off with a little summary; and so the very necessary, and more ignoble, aim is achieved. But is this time-honoured method of scientific publication really the best way to advance science? Moreover, in these stringent post-war days can we really afford, both financially and scientifically, to continue unaltered in our pre-war habits? Can we expect to publish our papers of pre-war length in pre-war style, or must we recognise that times have changed and modify our scientific ways and our journals accordingly?

What, after all, is the fate of a technical research paper in, let us say, botanical science? So many papers are published that it is a sheer impossibility to read more than a tithe. Moreover, the various aspects of botany are so specialised that the memoirs from one branch are not very intelligible to workers in other branches, or at the best do not arouse any great enthusiasm. In consequence, when a journal is published, many botanists just glance through the contents (who has not done this?), and if nothing catches their eye the journal is returned to the shelf; others read the summaries and are content; whilst a few, a very few, read carefully through the entire journal, or, more usually, through some particular memoir. Again, a morphologist reading, shall we say, a physiological paper is often lost in the data and formulæ, and longs for a clear-cut statement of what it is all about; and a physiologist reading the same memoir sighs sceptically over the abbreviated tables and graphs, and wishes to see more of the original figures and experimental details. The position is, in fact, that for the great majority of us all that is really necessary or useful is a very full summary or a *précis* of the paper—unless we happen to be one of the dozen or so investigators of like or cognate problems, when what we really need is a much more detailed presentation of the original matter. The present method, trying to please everybody, satisfies no one.

Now what I would say is this. Let us face reality, and let us quite frankly recognise—as sooner or later we shall be compelled to recognise—the financial limitations of our pockets and the space limitations of our periodicals. If we are to cater for the specialist, let us do it properly and write up our investigations in great detail with full original data, both negative and positive. Then very few researches can be published, for a journal could contain only one or two; and we should depend for our general literature, as of course we do now, on abstracting journals. If we are single-minded, and consider only the progress of knowledge, there is much to be said for this plan, for most of us admit that two-thirds of the scientific papers published are merely records of time spent, and have no permanent, and little (if any) temporary, value in the advancement of learning.

The alternative plan, and perhaps the more feasible, is to retain the scientific journals for the general scientific public and delete the long and very technical portions of the memoirs. The journals would then contain a number of very full summaries, the real essence of the studies, with the minimum of original data necessary for their comprehension. This would mean that the essential results of investigations could be produced much more rapidly, and this, with the volume of manuscript awaiting publication, would be advantageous; and, secondly, that the results would become much more widely known, being more readable. Further, in the majority of cases, owing to the elimination of expensive tables and plates (in any case most plates are, except in very special cases, a sheer anachronism and luxury in these days), the costs of production would be reduced by more than one-half, and this—there is no use blinking the fact—is for most of us a very material consideration indeed.

With regard to the full results of the investigations for specialist purposes, I would suggest that these be written up in the greatest detail, incorporating the essential working notes, so that anyone repeating the work could find in the memoir every required datum. These memoirs, together with the original drawings and photographs, type-slides, and specimens, would then be filed for reference in a kind of Somerset House for scientific records. This might be a central institution, as the British Museum, or preferably a decentralised scheme would be adopted, and botanical memoirs filed at Kew, zoological ones at the Natural History Museum, chemical ones at the Institute of Chemistry, and so forth. Very important papers much in demand might be mimeographed, or duplicated by photography or some other cheap process, and copies purchased at cost price by the principal research laboratories, or in special cases the originals could be sent to responsible investigators. These and many other elaborations could be adumbrated.

The above is the merest suggestion of what I cannot help thinking is a feasible scheme that would go

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